Reply to Referee 1

The authors would like to thank Dr. Cairo for their thoughtful and helpful comments and suggestions. His review has made a significant contribution to the improvement of the paper. The line numbering in the reviewers' comments refers to the manuscript published in AMTD whereas the line numbering in the responses refers to the new version of the manuscript.

Comment: (3,13) To my knowledge, laser commonly used in lidar practice are often guaranteed with linear polarization not better than 100:1, so, as reported, sometimes a polarizing cube is used to further purify the laser light polarization before the transmission into the atmosphere. However, this does not prevent problems arising from possible misalignments between the laser polarizing plane and the polarizing splitter incident plane. Maybe it is worthwhile noting that the two effects are inherently different, and can in principle be corrected differently, in one case by further filtering the laser light to remove the unpolarised residuals, in the other, by a proper alignment of the two (polarization and incident) planes. However, the authors' formalism is correct, and general.

Answer: We agree that the problem of the "polarization purity" of the laser should be discussed in more detail, Furthermore, we assume that the main contributor to a possible elliptical polarization of the emitted laser beam is the emitter optics and neglect the contribution of the laser itself. We changed the paragraph (page 3, lines 8-17) as follows:

The specified "polarization purity" of lasers commonly used in lidars is on the order of 100:1, if it is specified at all. Already the terminology indicates that such specifications are rather vague. Actually, laser manufacturers do not measure the state of polarization of the laser beams, and seem to give values which are under all circumstances on the safe side. Theoretically, nonlinear crystals as second and third harmonic generators should provide very clean linear polarization, just depending on the quality and accuracy of alignment of the crystals. Due to lack of detailed information we neglect this errors source in this work. However, in order to remove this uncertainty, in some lidar systems high quality polarizing beam splitters are used to improve the degree of linear polarization of the emitted laser beams. In both cases, the plane of polarization of the laser beam can be rotated by angle α with respect to the incident plane of the polarizing beam splitter in the receiver optics, which results in the Stokes vector $\boldsymbol{I}_L$ of the emitted laser beam:

Comment: (7,5-6) This claim should be substantiated or referenced.

Answer: The explanation in the original manuscript was based on the wrong parameter. It is not the combined retardation of the cleaned PBS that is the main error source, because it is assumed that the extinction ratio of the cleaning polarizing sheet filters is sufficiently small so that the cross-talk can really be neglected. However, in case the polarizing sheet filters are not aligned well with the PBS, their effect is reduced. We rephrased the paragraph (page 7, lines 10-12) accordingly:

We assume that the extinction ratio of the cleaning polarizing sheet filters is sufficiently small and that they can be oriented with an accuracy much better than ±5° with respect to the PBS, and that therefore the resulting error of $D_S^{\#}$ can be neglected (See Appendix).

and added and appendix (page 20) which quantifies the reduction and the consequential required alignment accuracy.

From the Müller matrix of two rotated diattenuators (see Freudenthaler, 2016; supplement Eqs. S.10.10.1)

$$\frac{\langle \boldsymbol{M}_A(\phi)\boldsymbol{M}_S|}{T_A T_S} = \langle 1 + c_{2\phi} D_A D_S \quad D_S + c_{2\phi} D_A \quad s_{2\phi} D_A Z_S c_S \quad s_{2\phi} D_A Z_S s_S |$$

we get the p- and s-polarised transmissions T^p and T^s

$$\frac{T_{AS}^p}{T_A T_S} = \left(1 + c_{2\phi} D_A D_S\right) + \left(D_S + c_{2\phi} D_A\right) = (1 + D_S)\left(1 + c_{2\phi} D_A\right)$$

$$\frac{T_{AS}^{s}}{T_A T_S} = \left(1 + c_{2\phi} D_A D_S\right) - \left(D_S + c_{2\phi} D_A\right) = (1 - D_S)\left(1 - c_{2\phi} D_A\right)$$

and the extinction ratio ρ_{AT} for the transmitted path

$$\rho_{AT} = \frac{T_{AT}^{s}}{T_{AT}^{p}} = \frac{(1 - D_T)\left(1 - c_{2\phi} D_A\right)}{(1 + D_T)\left(1 + c_{2\phi} D_A\right)} = \rho_T \frac{1 - c_{2\phi} D_A}{1 + c_{2\phi} D_A} = \rho_T \frac{\left(T_A^p + T_A^s\right) - c_{2\phi}\left(T_A^p - T_A^s\right)}{\left(T_A^p + T_A^s\right) + c_{2\phi}\left(T_A^p - T_A^s\right)}$$

$$= \rho_T \frac{(1 + \rho_A) - c_{2\phi}(1 - \rho_A)}{(1 + \rho_A) + c_{2\phi}(1 - \rho_A)} = \rho_T \frac{1 - c_{2\phi} + \rho_A\left(1 + c_{2\phi}\right)}{1 + c_{2\phi} + \rho_A\left(1 - c_{2\phi}\right)} = \rho_T \frac{tan^2\phi + \rho_A}{1 + \rho_A tan^2\phi}$$

$$\approx \rho_T \rho_A \left(1 + \frac{tan^2\phi}{\rho_A}\right)$$

Please note that p- and s-polarisations are with respect to the incidence plane of the polarising beam-splitter cube MS and that the polarising sheet filter is rotated by an angle of 90° in the reflected path, with cos(2·(90° + ϕ)) = cos(2ϕ). The extinction ratio ρ_{AR} for the reflected path can be derived in the same way

$$\rho_{AR} = \frac{T_{AR}^{s}}{T_{AR}^{p}} = \rho_R \frac{tan^2\phi + \rho_A}{1 + \rho_A tan^2\phi} \approx \rho_R \rho_A \left(1 + \frac{tan^2\phi}{\rho_A}\right)$$

For a typical polarising beamsplitter cube with T_T^p = 0.95, T_R^s = 0.99 and T_T^s= 1 – T_R^s, T_R^p = 1 – T_T^p, the extinction ratio in the transmitted path is $\rho_T = T_T^s / T_T^p = 0.0105$ and in the reflected path $\rho_R = T_R^s / T_R^p = 0.0505$. Using additional cleaning polarising sheet filters with $\rho_A = 0.01$, the combined extinction ratios $\rho_{AS} = \rho_A \cdot \rho_S$ are improved by a factor of 100. A misalignment of the polarising sheet filter by an angle ϕ with $tan^2\phi = \rho_A$, for example, decreases the improvement by a factor of two, which is in this case about ϕ = 5.7° (for $\rho_A = 0.01$).

Comment: (7,18-20) What follows is the crucial point of my review. I think that the sentence reported in the text understates what, to my opinion, is one cause of concern about the accuracy of all the absolute calibration techniques of the lidar signal which have been proposed so far. The reference to Bravo-Aranda et al., refers to the stability of photomultiplier gains over long times, but I think it is not sufficient to guarantee that. I hope that, if I behave myself, after my departure I will find myself in a place where the sensors' responses to signals are linear along their entire dynamic range. Unfortunately, it is well known that photomultipliers are far from heaven, both when used in photoncounting mode, or in current mode. The lidar return may be a more or less significant part of the total signal detected by the photomultiplier, depending on the altitude where it originates and, more significantly, on the sky background that can vary over several orders of magnitude. This means that the whole lidar returns are located on different portions of the photomultiplier response curve, in dependence of the sky background. These different portions can be locally linear, or quasi-linear, but may not share the same linearity. In other words, depending on the sky background, the photo impulse height spectrum of a photomultiplier can change substantially, thus affecting both photoncounting and current mode of detection. This effect may be dramatic or negligible, depending on the photomultiplier type, polarization, single realization of the device, and so on. Of course, if the effect is there, it has an impact on the absolute calibration, that became dependent on the sky background conditions. I am not aware of any study that focused on the dependency of the absolute calibration on sky background conditions, but in my experience as a researcher, I saw changes of volume depolarization values by few percents, simply induced by the sun rising or setting, so I am quite sure this effect can be present, even if it can be reduced or suppressed by an accurate choice of the photomultiplier type, polarization and amplification circuitry, lidar spectral bandwidth and so on. I am not saying that this effect is spoiling the results of this study, or the whole absolute calibration procedures. What I am saying is that the assumption of a constant photomultiplier gain is quite a severe one, and should be acknowledged as that.

Answer: we completely agree with the referee about the importance of the linearity of the photomultiplier response and the possible effects on the signals and the calibration. We also conclude with the need for investigating each lidar setup to exclude, or minimize and characterize such non-linearities.

Because our manuscript deals with the polarization dependent errors caused by the lidar emitter and receiver optics, it is not the place to additionally deal with electronic errors. We replace the sentence accordingly (page 7 lines 20-26):

The reflected and transmitted signals are detected by the photomultipliers which perform the light to electrical signal conversion. They affect the depolarization measurements as, in general, different photomultipliers have different gains. Regarding the Stokes-Müller formalism, we define the opto-electronic gains η_R and η_T for the photomultiplier gains of the transmitted and reflected signals including all optical attenuation of the lidar system in the transmitted and reflected path that is independent of polarization. We set them equal to 1 since we only investigate the polarization dependent errors of the lidar optics.

Comment: (9,5) The relationship between depolarization ratio and particle "asphericity" (whatever that means) is not so straightforward. Even under the simplified assumption of particles as oblate or prolate spheroids - unrealistic, but widely used because it allows analytical expression for the scattering equations solution - one could find greater depolarization ratio for aspect ratioes close to unity. The authors might drop that sentence, or quote some reference to T-matrix computations, as instance.

Answer: the sentence has been removed.

Comment: (12,6-7) "do not use laser emitting optics if possible" please rephrase, as in this form, it is not clear what it is meant, at first sight.

Answer: the phrase has been rewritten (page 12, line 14).

It is recommended to emit the laser beam directly to the atmosphere to avoid this error source

Comment: (16,3) It may be worthwhile to note here that the opposite result applies when a relative calibration approach is pursued, i.e. when the theoretical value of the molecular depolarization is imposed in a region of the lidar profile which is free of aerosol. In that case, the instrumental effects here discussed lead to an underestimation of the aerosol depolarization.

Answer: We don't want to consider a relative calibration at all in this manuscript, because the involved uncertainty is difficult, if not impossible, to estimate accurately enough and in general unacceptably high. Furthermore, it can be seen from Figs. 9 and 10 that in some cases the distributions are shifted to lower values. Therefore we think that the limited number of lidar set-ups considered in this manuscript does not allow to state a general overestimation and removed the corresponding sentences.